DOCUMENT RESUME

ED 478 330 CE 083 412

AUTHOR	Jacobson, Louis; Petta, Ian
TITLE	Measuring the Effect of Job Service Referrals and Placements in Washington and Oregon.
INSTITUTION	Westat, Inc., Rockville, MD.
PUB DATE	2001-06-00
NOTE	32p.; Paper presented at "Making Connections in the New Economy,"America's Workforce Network Research Conference (Washington, DC, June 26-27, 2001).
AVAILABLE FROM	For full text: http://wdr.doleta.gov/conference/pdf/jacobson.pdf.
PUB TYPE	Reports - Evaluative (142) -- Speeches/Meeting Papers (150)
EDRS PRICE	EDRS Price MF01/PC02 Plus Postage.
DESCRIPTORS	Benchmarking; Cost Effectiveness; Dislocated Workers; Employment Experience; Employment Level; Employment Opportunities; Employment Patterns; *Employment Services; *Job Placement; Program Effectiveness; *Quasiexperimental Design; Reentry Workers; *Referral; Research Methodology; *Research Problems; Research Utilization; Sampling; Surveys; Unemployment; Unemployment Insurance
IDENTIFIERS	*Job Service; Job Training Partnership Act 1982; Oregon; Washington

ABSTRACT

The utility of a quasi-experimental evaluation design for estimating benefits derived from a program to match job-seekers to openings listed by employers was examined in three studies of Job Service referrals and placements in Washington and Oregon. Data examined included a mail survey of a sample of 587 Washington residents referred to job openings in the first half of 1998 and records of 328,815 spells of unemployment experienced by unemployment insurance (UI) claimants in 1987-1995. The effectiveness of job service placement services and referral services were estimated using program cost-benefit ratios, duration of joblessness, success in obtaining interviews, and successful job placement for job-seekers with strong and spotty work records. An expert panel examining the methodology concluded that: 1) survey results were imperfect because of the possibility that some job-seekers who failed to secure interviews were screened out by employers; 2) the sample was small and may not have been representative; 3) improving the survey and using telephone follow-up should overcome the problems; and 4) the design holds promise for obtaining credible estimates of the value of being placed that also can be used as benchmarks in assess the bias in non-experimental estimators. (Contains 11 tables.) (MN/CG)

Reproductions supplied by EDRS are the best that can be made
from the original document.

U.S. DEPARTMENT OF EDUCATION
Office of Educational Research and Improvement
EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)
This document has been reproduced as received from the person or organization originating it.
Minor changes have been made to improve reproduction quality.
Points of view or opinions stated in this document do not necessarily represent official OERI position or policy.

PERMISSION TO REPRODUCE AND DISSEMINATE THIS MATERIAL HAS BEEN GRANTED BY
L. Jacobson
TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)

Measuring the Effect of Job Service Referrals and Placements in Washington and Oregon

by
Louis Jacobson and Ian Petta
Westat, Inc. 301 251-8229 15 June 2001

ED 478 330

1.0 Introduction

This paper describes our research using data from Washington State and Oregon to measure the benefits derived from the principal labor exchange service offered by the Job Service: matching job-seekers to openings listed by employers. Most matching is done by job-seekers looking through computerized lists at Job Service offices. Job-seekers can also view listings using terminals at other public places such as libraries and at home using the Internet.

Sometimes Job Service staff look through listings with individual job-seekers. They also assist employers by looking through job-seeker applications to fill specific openings. Those identified then are contacted by staff or through automated phone notifications systems and encouraged to apply for those jobs. In a few states, including Washington, systematic computerized matches are made weekly between job-seeker registrations and all job openings, and then job-seekers are notified that a match has been made.

At one time job-seekers were referred to employers exclusively by the Job Service staff. This system still is widely used in Washington. However, many states make it possible for job-seekers to obtain contact information through the computer with no staff intervention. Oregon uses this system extensively. However, it is the only state that asks job-seekers to identify themselves prior to obtaining the contact information. Thus, administrative records in both Washington and Oregon are able to track referrals to specific openings.

For the nation as a whole the Job-Service was credited with referring 7 million different job-seekers to jobs in 1999, out of 17 million registrants. Of this group, 2.3 million were placed at the 7.3 million jobs listed with the Job Service.

However, these statistics probably substantially underestimate the number of people referred and placed by the Job Service because it is has become increasingly difficult for the Job Service to track referrals made without staff intervention and the subsequent placements. Evidence that tracking of

CE 083 412

BEST COPY AVAILABLE

placements probably declined is that nationwide placements fell by 20 percent, but increased by 2 percent in Washington and Oregon, which track placements better than most states.

Table 1a displays the proportion of registrants receiving various forms of Job Service assistance in 1999. About 60 percent of registrants received some reportable service, with job referral being the most common service received. However, a substantial fraction received other forms of job search assistance including participation in workshops. Relatively small fractions of registrants received assessment services and referrals to services provided by other organizations.

Table 1a. Distribution of Services Received by Job Service Registrants

Reportable service	60.3%		
Referred to job		37.7%	
Placed			9.6%
Job search activity		33.9%	
Assessment service		9.2%	
Interviewed			6.1%
Counseled			2.7%
Tested			1.4%
Referred to other services		7.7%	
Referred to supportive service			7.7%
Referred to training or education			6.1%

Finally, it is instructive to compare the number of placements made by the Job Service to the number of individuals who enter employment by any means after participating in a program funded under the Job Training Partnership Act (JTPA). Table 1b shows that in 1999 the Job Service placed about 14 times more individuals than entered-employment following termination from a JTPA program, at less than one-tenth the cost per placement or entered-employment (to the Federal government). The statistics would look even more favorable to the Job Service if all PLX users who entered employment, not just those placed, were compared to JTPA participants who entered employment.

On the other hand, JTPA participants often have much poorer prospects for finding work on their own than individuals using PLX. Also, the cost statistics omit the substantial state funds that often go to the Job Service. Overall, state funds may equal as much as one-third of the Federal Wagner-Peyser share, and in Oregon they account for 50 percent of all funding. However, even when the comparisons are limited to individuals with similar employment prospects and all costs are taken into account the Job Service appears to effectively serve many more individuals at a much lower per-person cost.

Table 1b. Job Service Cost per Placements versus JTPA Cost per Entered-Employment

Job Service

Registrants	17,288,127
Referrals	7,008,534
Placements	2,304,772
Wagner-Peyser Allocation	$893,000,000
Cost per person referred	$127
Cost per person placed	$387

Job Training Partnership Act

	Economically Disadvantaged		Dislocated	Total
	Adults Title II-A	Youth Title II-C	Title III	
Terminees	202,300	85,000	240,900	528,200
Entered-Employment	151,600	72,000	240,900	464,500
JTPA Allocation	$952,500,000	$129,600,000	$1,080,400,000	$2,162,500,000
Cost per terminee	$6,284	$1,799	$4,485	$4,655
Cost per entered-employment	$4,708	$1,525	$4,485	$4,094

2.0 Measurement Issues

It is evident that the number of individuals helped by the direct placement services provided by the Job Service are substantially greater than the number helped through other Job Service aid and dwarfs the number helped through JTPA programs. What is not at all evident is how the per-person benefits vary across the "treatments" shown in table 1a.

There are several studies that provide accurate estimates of the value of job search assistance for dislocated workers, unemployment insurance (UI) claimants, and the economically disadvantaged, as well as the benefits of classroom vocational training and on-the-job training for these groups. Importantly, none of the studies explicitly examine the role of Job Service job matching, although much of the benefits of the various treatments probably come from participants' use of those services. Even studies of job search assistance focus on workshops usually lasting only a few days, and do not get inside the "black-box" to determine whether the source of the benefits is increased use of Job Service referrals or more effective finding of jobs through other means.

Despite their shortcomings with respect to the role of job-matching, these studies provide persuasive evidence about the net benefits of the treatments examined because they are based on experimental evidence derived from random-assignment designs. In broad outline, these studies require program operators to use normal channels to enlist more volunteers interested and qualified for participation than the programs can accommodate. They then randomly select a control group of individuals who are not permitted to participate in these programs for a set period.

Estimates of these programs' "value-added" are then obtained through straightforward comparisons of the post-program outcomes of the participants to what subsequently happened to members of the control group. Use of the random-assignment design ensures that whatever factors affect the decision to volunteer for a given treatment and also affect post-program outcomes are distributed identical between the targets (participants) and controls.

It has been well-documented that individuals volunteering to participate in government funded employment and training programs are drawn to the programs because they have unusual difficulty achieving their career goals. However, once factors associated with such difficulty are taken into account it often turns out that the volunteers differ from apparently similar non-volunteers because they have greater motivation to succeed, and therefore, would have had better outcomes even in the absence of participation.

Table 2 presents an example of how both negative and positive self-selection bias makes it difficult to draw accurate conclusions from simple comparisons among UI claimants who registered with the Washington State Job Service between 1987 and 1995 and changed jobs (rather than were recalled to former jobs or dropped out of the labor force).

Table 2. Probability of Claimants who Change Jobs Returning to Work

		Referred	
Return after:	Not Referred	Not Placed	Placed
1 quarter	49.0	41.7	63.5
2 quarters	67.3	66.8	86.2
3 quarters	78.2	86.2	92.4

As might be expected, in all three periods those placed at jobs were more likely to return to work than those referred-but-not-placed and those not-referred. However, in the first quarter following the start

of a period of joblessness, not-referred claimants were about 25 percent more likely to return to work than those who found jobs to which they were referred, but did not end-up being placed at those jobs.

This evidence suggests that within the first 13 weeks of unemployment individuals who did not use the Job Service were better able to find jobs on their own than those who did. Thus, there was negative selection on the part of Job Service users. However, the same comparison in quarter 3 suggests that Job Service users were about 10 percent more likely to return to work. This 10 percent increase could reflect better qualifications and more interest in finding work, or the benefits of using the Job Service.

Further, if we wanted to know how likely job seekers placed in the first quarter were to find jobs in the absence of use of the Job Service, would it be better to compare those placed to those referred or to those not referred? Without some type of randomization among those "treated" versus those not treated it is extremely difficult to determine what would have happened in the absent of a given treatment.

The primary challenge in conducting an analysis of the value-added of direct placement services is that, by law, the Job Service must help all comers, and by its very nature, it would be extremely difficult to prevent job-seekers from viewing listings. We, therefore, cannot use the random assignment technique that has been successfully employed in measuring the effectiveness of classroom training and job-search workshops, to assess the Job Service "treatment" of viewing listings.

What we were able to do, however, was employ a promising approach that takes advantage of a natural source of variation to measure the value of being placed relative to being referred-but-not-placed. This "quasi-experiential" approach underestimates the total benefits of using the Job Service because it omits the value of obtaining information from viewing the listings and pursuing promising openings that ends up helping these users quickly find and accept other jobs. However, once we have an accurate measure based on a quasi-experimental design, we can then use that measure as a benchmark for developing a non-experimental estimation procedure that produces accurate results.

3.0 Use of a Quasi-Experimental Design

The key to developing an unbiased measure of the value of placements relative to referrals-not-leading-to-placements is finding a naturally occurring source of random variation in the probability of

being placed following obtaining a referral to a particular opening. We felt that such natural variation was likely to occur because there was a lag between when an employer made a job offer to an opening listed with the Job Service and when that listing was removed from the Job Service computers.[1] As a result, some referred job-seekers applied to openings after they were filled, and therefore through no fault of their own, could end-up without job offers.

Applying to a job that is already filled is similar to the following experiment (that could not actually be conducted). Employers who list jobs with the Job Service would be required to request the Social Security Account Number (SSAN) of each applicant and ask how they heard about the job. If the job-seeker's SSAN ends with 2 and the person heard about the job from the Job Service the employer would be required to tell the job-seeker the job has already been filled.[2]

We set out to identify referred job-seekers who applied too late to obtain job offers by mailing surveys to 6,000 job-seekers in Washington State, half of whom were placed, and half referred to the same jobs, but not placed.[3] We used a variety of incentives to secure a maximal response, but only obtained a 22% response rate. Thus, our sample may not be representative of the universe, and created larger confidence intervals for the point estimates than we would like. Nevertheless, the sample was sufficient to test the viability of the approach.

Table 3 presents the main results of the survey. Column 1 describes the outcomes following obtaining specific referrals. To our knowledge this is the only information available on this topic.

---

[1] Lags exist because employers do not immediately notify the Job Service when a job-offer is made. Our own survey of job-seekers, interviews with Job Service staff, and interviews with employers suggested that it is not simply that employers are too busy to notify the Job Service, but that job-seekers often take some time to decide to accept on offer, do not show up for work after accepting an offer, and quickly stop working after they discover the job is not to their liking or more difficult than they anticipated. Thus, employers often find it useful to keep adding to their list of potential hires, even after an offer has been made. Also, many jobs are filled quickly (within a week or two of being listed). Thus, even short lags in their removal can generate referrals to filled jobs.

[2] As already noted, a better experiment would be to randomly deny access to the list of Job Service openings because then our measure would include benefits from viewing the lists and either not finding suitable opening or not obtaining a job from a suitable opening. Such an experiment is impossible, and because those benefits are omitted, our quasi-experimental design produces conservative estimates.

[3] A key difference between the natural experiment we used and the experimental design sketched above is that when a job-seeker is not granted an interview to one job (because it is already filled) he or she can gain interviews as a result of other referrals. To deal with this issue we compared placed individuals who were referred to the same job, but not placed at any job. We estimate that removing those subsequently placed only reduced the pool of those referred-but-not-placed by one-eighth.

28.1 percent did not attempt to secure an interview. 34.4 percent tried to obtain an interview, but were unable to do so. 38.5 percent secured an interview, but of this group, only 40.0 percent were placed. 32 6 per :ent were not given an offer, and 10.2 percent either rejected an offer, or accepted the offer and later decided not to report for work.

Table 3. The Distribution and Effect of Referral Outcomes.

Outcome	Percent of Job-Seekers	Change in unemployment duration (weeks)
1. Placed (started work)	15.0	-4.70
2. Accepted offer, no show	4.6	-1.25
3. Rejected an offer	5.6	-6.52
4. Interviewed, did not receive offer	12.2	1.65
5. Tried, but failed, to get an interview	34.4	--
6. Did not follow-up referral	28.1	-1.36
Groups 1 through 4 relative to 5	38.5	-2.50

We feel that a comparison between those who obtained interviews and those who tried but failed to obtain an interview provide evidence that is reasonably close to that which would be derived from a random-assignment design. We hold this view because we believe that most of these job-seekers who tried, but failed to secure an interview, applied for the jobs too late to get an interview, and therefore, comprise a quasi-control group.

Unfortunately, the purity of the quasi-control group is not certain. Some individuals could have failed to secure interviews because they were screened out by employers, and therefore, should not be included in the comparison group. In retrospect, we should have probed deeper to determine whether pre-screening occurred, and have subsequently designed a survey instrument that should be able to limit the comparison group to job-seekers who applied too late to obtain interviews.

Column 2 of table 3 shows the difference in weeks between the duration of joblessness of individuals in each category and those who tried but failed to obtain an interview. The results show that those placed returned to work 4.70 weeks sooner than those who tried but failed to obtain an interview. Those who rejected an offer returned to work 6.52 weeks sooner. Presumably, these rejections were due to having a better offer in hand or lined up. Also, much in keeping with expectations, individuals who failed to get an offer, took 1.65 weeks longer to find work.

On average, job-seekers who interviewed returned to work 2.50 weeks sooner than those who tried, but failed to obtain an interview. It is this finding that resembles the results of a true experiment.

In addition, to computing the effect for all job-seekers together we separately examined what happened to job-seekers who had "spotty" work records (employment in 0, 1, or 2 quarters in the year prior to the start of their unemployment spell), and job-seekers who had "strong" work records (employment in 3 or 4 quarters in the year prior to their unemployment spell).

The distribution of outcomes is very similar to those shown in column 1 of table 3 for both groups. Thus, are omitted from table 4. The effect of being placed (relative to being unable to secure an interview) also was similar for both groups. However, the difference in weeks of unemployment across the other outcomes is very different in all other cases.

Table 4. The Effect of Referral Outcomes for Job-Seekers with Spotty and Strong Work-Records.

	Spotty	Strong
1. Placed (started work)	-4.10	-4.22
2. Accepted offer, no show	-5.64	-2.66
3. Rejected an offer	-2.97	-7.35
4. Interviewed, did not receive offer	4.29	0.44
5. Tried, but failed, to get an interview	--	--
6. Did not follow-up referral	-3.02	2.62
Groups 1 through 4 relative to 5	-1.17	-2.77

On average, job-seekers with spotty records who interviewed for a referred job returned to work 1.17 weeks sooner than job-seekers who tried but failed to obtain an interview. Job-seekers with strong work records who interviewed returned to work 2.77 weeks sooner than job-seekers unable to secure interviews.

The substantially greater effect of being interviewed for those with strong versus spotty work records mainly stems from the those with strong records who failed to obtain an offer returning to work only slightly later than those unable to secure an interview. In contrast, those with spotty work records who failed to obtain an interview returned to work over 4 weeks later than those unable to obtain an interview.

The large difference in unemployment duration between those with spotty work records who did not obtain offers versus those unable to obtain interviews suggests that being screened out by one employer makes it likely that other employers also would not hire the job-seekers with spotty work records. This result suggests that including individuals in our quasi-control group who were screened out by employers prior to being offered a chance to interview could lead to substantial overestimation of the effect of being placed for this group.

In sharp contrast, the small difference between those with strong work records who did not receive offers and those unable to secure an interview suggests that being screened out by employers does not have much of a negative effect on the duration of job search. This finding suggests that even if some of those who were unable to secure interviews were screened out by employers it would lead to a small overestimation of the true effect for individuals with strong work records. This is an important finding because it suggests that the measures developed from our survey for job-seekers would be relatively unaffected even if there were substantial numbers of job-seekers screened out by employers among those unable to secure an interview.

It also is worth noting that our initial expectation was that the effect of referrals would be smaller for workers with strong work-records than those with spotty work-records. Our reasoning was that workers with strong-records would have better access to information sources, such as leads from friends and relatives, that would help these individuals more quickly finding suitable jobs without Job Service assistance. However, it appears that this hypothesized lack of information sources is more than offset by job-seekers with spotty work-records being willing to accept a far greater range of jobs, most of which require little training or experience.[4]

3.1 Use of Quasi-Experimental Estimates as Benchmarks

The final exercise we conducted using the survey data was to compare the results based on a quasi-experimental design to those that would be generated using non-experimental estimators. The non-

[4] If we are correct that job-seekers with strong work-records are looking at a narrow range of jobs, than it suggests that it is important to compare placed job-seekers with strong work records to similar job-seekers who are referred but not placed at those same jobs. Also, it suggests that for job-seekers with strong work records it is relatively unlikely that there will be a lot of suitable openings. Thus, our inability to prevent job-seekers applying too late to be placed at one job from gaining interviews from other referrals is unlikely to have a large effect on the estimates.

experimental estimator we used compares the difference in the duration of joblessness between job-seekers who are placed (those in group 1 in tables 3 and 4) versus all those who are referred but not placed (those in groups 2 through 6).

In the absence of detailed survey information about what happened following the receipt of a referral this is the only meaningful comparison that can be made. A regression is used to hold constant the observed differences between those placed and those referred but not placed.

To create a comparable quasi-experimental estimate we assume that the effect of the referral on those not placed is zero, and that the only effect is on those placed. Doing this simply requires weighting the estimates displayed in tables 3 and 4 by the proportion of interviewed individuals who are placed. Fortunately, precisely the same assumption about the value of being referred-but-not-placed equaling zero underlies those estimates. As a result, both estimates are comparable, and both do not include any benefits stemming from viewing listings, but not being placed.

Line 1 of table 5 shows the quasi-experimental estimates and line 2 shows the proportion of job-seekers who are placed following an interview. Dividing the estimates on line 1 by the proportions on line 2 produce the quasi-experimental estimates shown on line 3 (based on the assumption that there is no value-added from viewing listings unless they lead to a placement).

Table 5. Comparisons between Quasi-Experimental and Non-Experimental Estimates

	Work-Record:		
	Spotty	Strong	Both
Quasi-Experimental Result			
1. Interviewing and possible placement versus trying, but failing to secure an interview	-1.082	-2.761	-2.383
2. Fraction of those interviewing who are placed	.380	.368	.374
3. Placement effect (line 1 / line 2)	-2.849	-7.510	-5.978
Non-Experimental Result			
4. Placement versus referral but no placement	-3.411	-4.496	-4.046
Difference quasi-experimental versus non-experimental			
5. Weeks	.563	-3.013	-1.933
6. Percent of non-experimental result	-16.5%	67.0%	47.8%

Line 4 of table 5 shows the non-experimental estimate based on comparing those placed to those referred-but-not-placed. Line 5 shows the difference between the two estimators in weeks of unemployment and line 6 shows the difference as a percent of the quasi-experimental estimator.

The key conclusions from this table is that for job-seekers with strong work records the non-experimental estimates are about 67 percent less than the quasi-experimental estimates, and for job-seekers with spotty work records the non-experimental estimates are about 17 percent greater than the quasi-experimental estimates.

Our view is that this comparison provides a useful assessment of the bias in the non-experimental estimates for job-seekers with strong work-records. We hold this view because we doubt that including some individuals in the quasi-control group who were screened-out by employers had much of an effect on the quasi-experimental results.

In contrast, we feel that is far more difficult to use the quasi-experiential results to assess the bias in the non-experimental estimator for job-seekers with spotty work-records. As previously discussed, the large effect of not getting an interview for this group makes it far more likely that the quasi-experimental results themselves over-estimate the true effect.

Nevertheless, we are inclined to accept both the much smaller effect of placements on job-seekers with spotty work records and that the non-experimental estimators tend to over-estimate the true effects. Both these surmises stem from the estimate that job-seekers with spotty work-records are unemployed 3.02 weeks less than those who fail to get interviews and only 1.08 weeks longer than those who are placed.

More specifically, it is our view that the small difference between being placed versus not following-up the interview arises because job-seekers with spotty work-records (who use the Job Service) have access to other sources of leads that are almost as good as those available from the Job Service, and therefore, the value-added of the Job Service placements should be relative small.

3.2 Summary of the Survey Results

An expert panel reviewed our survey results and agreed on four main conclusions:

1. The survey results are imperfect because we did not rule out the possibility that some job-seekers who tried but failed to secure interviews were screened out by employers.

2. The sample may not have been representative of the universe of placements and was small leading to large confidence intervals surrounding some point estimates.

3. These problems should be able to be overcome by improving the survey instrument to determine if employers pre-screened job applicants and by using a telephone follow-up to obtain a larger and more representative sample.

4. Thus, in contrast to earlier efforts, our quasi-experimental design holds substantial promise for obtaining credible estimates of the value of being placed that also can be used as benchmarks in assessing the bias in non-experimental estimators.[5]

Also, despite the above shortcomings, the survey directly provided useful information about:

- Why only about 15 percent of job-seekers referred to an opening are placed at that job.

- How long it takes job-seekers with different referral outcomes to return to work.

- How the outcomes and the duration of joblessness differ between job-seekers with strong work-records (3 or 4 quarters with employment prior to becoming unemployed) versus spotty work-records (less than 3 quarters with employment).

This information, in turn, made it possible to compare two alternative ways to estimate the effect of placements stemming from Job Service referrals on the duration of joblessness, and assess the accuracy of the two procedures:

- The quasi-experimental design compared job-seekers who tried, but failed to secure interviews to those who succeeded in gaining interviews. On average, this design produced estimates that interviewing for a referred job reduced joblessness by 2.76 and 1.08 weeks for job-seekers with strong and spotty work records, respectively. If we assume that only those placed benefited from gaining interviews, on average this design produced estimates that placements reduced joblessness by 7.51 and 2.37 weeks for job-seekers with strong and spotty work records, respectively.

---

[5] The expert panelists also were skeptical that the effects of placements and referrals were so much larger than estimates of the effects of job search assistance (JSA) generated from random-assignment designs. However, it is our view that this criticism stemmed from key differences between the treatment offered by the Job Service and the JSA treatments analyzed using random-assignment designs. Those studies tended to examine the effect of participation in a workshop that usually lasted well less than 5 days.

We suspect that the positive effects were narrowly distributed among a small segment of participants who used the information they obtained to change the way they would otherwise have searched for work. However, the design averaged the effect across all participants. In contrast, our approach assumed the entire effect was on the 15 percent who were placed.

In addition, as will be discussed in the next section, the period over which job-seekers actively used the Job Service to look at openings was much longer than the duration of the workshops. Thus, we believe the treatment provided by the Job Service should be expected to produce much larger results than a one-week workshop that was not followed-up with additional actions by many participants.

- The non-experimental design compared job-seekers who were placed to those who were referred, but not placed. On average, this design produced estimates that placements reduced joblessness by 4.50 weeks and 3.42 weeks for job-seekers with strong and spotty work records, respectively.

- It is our view that the quasi-experimental estimate is relatively free of bias for job-seekers with strong work-records. Because individuals who interviewed and did not receive job offers are unemployed only .44 weeks longer than members of the quasi-control group there appears to be little correlation between not obtaining an offer and subsequent unusual difficulty finding work. This makes us believe that even if employers are screening out substantial numbers of the quasi-control group (who tried but failed to obtain interviews), it would not have much effect on that groups jobless duration.

- Based on the same reasoning we have more doubt that the quasi-experimental estimates for job-seekers with spotty work-records are relatively free of bias. For this group, job-seekers who failed to get offers were unemployed 4.29 weeks longer than members of the quasi-control group. Thus, if those being screened-out prior to being interviewed had similar durations to those screened-out after being interviewed, large over-estimates would occur if employers screened out many job-seekers prior to being interviewed.

- Because we feel that the quasi-experimental results are relatively free of bias, we believe that the non-experimental estimator substantially under-estimates the true effect. However, we would prefer to have had a larger and more representative sample on which to base this assessment.

Overall, the authors of this report and the expert panelists agree that obtaining unbiased estimates from a quasi-experiment design is crucial to accepting the validity of results derived from any non-experimental design. We also agree that the quasi-experimental evidence we generated for job-seekers with spotty work records is of limited use for assessing the value of placements or judging the validity of non-experimental estimators.

We disagree, however, on how much confidence can be attached to our quasi-experimental point estimates for job-seekers with strong work records and our conclusion that the non-experimental estimates under-estimate the true effect for that group.

Basically, the panelists felt that the main contribution of our work was to demonstrate that it was highly worthwhile to improve the survey to the point it produced highly credible estimates. Their view was that only improved results should serve as benchmarks for judging the direction (and magnitude) of the bias in non-experimental estimates.

We regard having the expert panelists agree that it is possible to produce credible estimates using our quasi-experimental design as a considerable achievement. However, we also feel that the existing evidence is sufficient to believe that the non-experimental estimates of per-person placement effects for claimants represent conservative estimates of those effects. We, therefore, feel that is worthwhile to present our results for this group in the next section.[6]

We even more strongly hold the view that assuming referrals that do not lead to placements have no value leads to a highly conservative estimate of the total value of referrals. Thus, we feel that it worthwhile to present the benefit-cost estimates in section 5.

4.0 Non-Experimental Estimates Using Administrative Data

This section describes our analysis of the effects of referrals and placements made to unemployment insurance (UI) claimants in Washington and Oregon. These estimates used non-experimental design with exceptionally large administrative databases.

We restricted the analysis to claimants because:

1. Claimants must attest that they did not quit their former jobs and that they are actively searching for work.

Also, the database is unusually rich for claimants. Thus, we can determine:

2. What labor exchange services were received and how many weeks of joblessness preceded their receipt.

3. How many quarters of unemployment followed obtaining a given referral.

4. Whether a claimant took a new job, rather than ended a spell of unemployment by returning to work with the employer that laid him or her off, or dropped out of the labor force.

Thus, these databases allow us to compare placed claimants to other claimants who are looking for new jobs, and are similar in other key respects, including whether and when they obtain referrals.

[6] The experts differed in the emphasis they placed in the usefulness of having these estimates versus estimates that could be much improved. Thus, taken together, there is a paradoxical flavor to the reports in that they suggest both that the estimates we produced are the best available and that they are not good enough to be taken as reasonably close to the true values.

Conceptually, we expect that selection bias will be greatly reduced by controlling for choosing to use labor exchange services, finding openings to which the person wants to be referred, and being jobless for a comparable period at the time of referral. Together with controlling for age, sex, prior earnings level, tenure, occupation, industry, and local labor market conditions we find it quite plausible that our comparisons are being made among job-seekers with similar reemployment prospects.

In addition, most claimants must register with the Job Service, but are not required to use the Job-Service to search for work. Thus, we have information about a comparison group of claimants who did not obtain referrals that can be used to measure the benefits of obtaining referrals that do not lead to placements. Self-selection bias is more likely to be present in comparisons between those referred and those who chose not to look at listings, than between those placed and referred. However, we think that the control variables we have available to us should substantially reduce such bias. In particular, we believe that comparing claimants who are referred to claimants who are not referred but have similar durations of joblessness should go a long way to reducing selection and other sources of bias.

4.1 Measures of Placement and Referral Effects

Table 6 describes our estimates of the effect of being placed and being referred in each of five periods relative to the start of claimants' spells of joblessness. As noted in the preceding subsection, we feel that confining the comparisons to claimants who were unemployed for the same duration at the point services were received should substantial reduce selection bias. We also controlled for a host of other factors. (Appendix A describes the variables included in the estimating equations, and displays the results of using our model)

In Washington, we estimate that, on average, each referral not leading to a placement reduced joblessness by 2.149 weeks and each placement reduced joblessness by 9.867 weeks.[7] Referral effects rose slightly peaking in weeks 10-13, and then declined. Placement effects rose sharply over the first three periods, and also peaked in weeks 10-13, but only declined slightly thereafter.

[7] We measured joblessness using quarterly wage records because once claimants exhausted UI benefits we could no longer use UI payment measures alone, and many claimants would have exhausted benefits in the absence of being referred. Rather than use a hybrid measure of weeks prior to exhaustion and quarters times 13 after exhaustion we determined that multiplying quarters times 13 and adding 4 closely correlated with weeks of unemployment during the pre-exhaustion period.

Table 6 Estimates of Referral and Placement Effects

Referral/Placements made in:	---- Effect on weeks of joblessness ---- referrals (relative to no referral)	placements (relative to no referral)	placements (relative to referral no placement)
A. Washington 1987-95			
week 1	-2.029	-5.569	-3.540
weeks 2-9	-2.340	-8.427	-6.087
weeks 10-13	-2.460	-12.388	-9.928
weeks 14-18	-2.175	-11.867	-9.692
weeks 19-26	-1.399	-12.187	-10.788
average of all weeks	-2.149	-9.867	-7.718
B. Oregon 1995			
week 1	-2.292	-5.499	-3.207
weeks 2-9	-0.171	-5.223	-5.052
weeks 10-13	-2.781	-6.943	-4.162
weeks 14-18	-2.550	-5.981	-3.431
weeks 19-26	-1.293	-9.236	-7.943
average of all weeks	-1.131	-5.751	-4.620

C. Incidence of Referral and Number of Claimants in our Sample

	Percent of Claimants Referred		% Claimants Remaining	
	Oregon	Washington	Oregon	Washington
week 1	2.9%	2.3%	100.0%	100.0%
weeks 2-9	12.0%	4.6%	83.6%	89.0%
weeks 10-13	4.6%	2.8%	42.9%	51.4%
weeks 14-18	4.2%	3.4%	32.9%	40.6%
weeks 19-26	5.5%	5.0%	24.0%	29.8%
Average of all weeks			Number in sample	
	5.9%	3.4%	138,280	328,815

Note: The Washington sample includes 20 percent of all claimants who changed jobs. The Oregon sample includes 100 percent of those claimants. Claimants who return to a former employer or do not show any employment within 7 quarters of the start of unemployment spells are not included in either sample. Also, we excluded referrals and placements that occurred after the claimants stopped collecting benefits or reached their 26th week of unemployment (when most claimants exhaust their benefits). Thus, our samples only include "active" claimants.

The placement trend indicates that claimants are able to far more readily find jobs by other means close to the start of their jobless period, but as they exhaust the most promising leads, it becomes increasingly more difficult to find jobs without Job Service referrals. Similarly, it is our view that the decline in the value of referrals not leading to placements stems from job-seekers first use occurring in an earlier period and the value of the information gained by viewing listings and contacting employers but not being placed declining with subsequent use. Thus, placements become increasingly more important as jobless duration lengthens.

The increase in the effect of placements relative to referrals is shown in column 3 of table 6, which displays the difference between the placement and referral effects. We also regard these figures as conservative estimates of the placement effects based on assuming that the entire referral effect is capturing selection and other measurement biases.

For Oregon, we estimate that, on average, each referral not leading to a placement reduced joblessness by 1.131 weeks and each placement reduced joblessness by 5.751 weeks. In both cases the effects are substantially less than those measured for Washington. However, the pattern of referral effects are similar to those in Washington except during weeks 2-9. The pattern of placement effects are not especially similar accept for week 1 and weeks 19-26.

We examined a number of explanations for why the effects are different in the two states. We believe that some of the difference is due to the Washington data covering a period where jobs were difficult to find and extended benefit programs were in place. As a result, in the absence of placement, unemployment would be much longer. However, we also believe that differences in requirements placed on Washington and Oregon claimants and funding to ensure the requirements were met were of great importance. More specifically, Oregon used its own funds to call-in many more claimants to be interviewed by Job Service staff to ensure they are complying with the UI work-test. We believe that once an office visit is made it is natural for claimants to look at job orders and obtain referrals.

Panel C of table 6 confirms that a much larger fraction of claimants are referred in Oregon than in Washington. The difference in favor of Oregon is especially large during weeks 2-9, when mandatory registration is required and screening is most common. In contrast, the difference is smallest in weeks 19-26 when it is most likely claimants in both states are using the Job Service because other methods to locate jobs have failed. Thus, we feel that is not at all coincidental that the biggest differences between

the two states occurred in weeks 2-9, when usage differed the most, and the smallest differences occurred in weeks 19-26, when usage differed the least.

If our hypothesis is correct that Oregon's UI collection requirements led many more claimants to begin using labor exchange services early during their unemployment spell and feel more pressure to assiduously search for work or loose UI benefits; then it is reasonable to expect that Washington claimants who take the trouble to use the Job Service are more in need of help, and therefore, likely to show greater benefits per-person. Similarly, it is reasonable to expect that Oregon claimants who do not obtain referrals search harder by other means, and thus, reduce the measured difference between users and non-users of labor exchange services.

Our data do not prove that UI rules lead to claimants returning to work or ending their spell more quickly in Oregon than Washington. However, it is clear that claimants in Oregon return to work more quickly. As shown in columns 3 and 4 of panel C, over 5 percent fewer Oregon claimants who were unemployed for 1 week continue into the 2nd week. Also, only 42.9 percent of Oregon claimants continue into the 10th week, compared to 51.4 percent of Washington claimants.

Finally, what ever the reason for the quicker return to work and earlier use of the Job Service in Oregon, this pattern makes it is more difficult to control for the effect of unobserved characteristics. This is the case because elapsed duration is one of the most powerful predictors of "what otherwise would happen". Thus, we believe that the affect of selection bias leads to greater underestimation of the true effect in Oregon than in Washington.

4.2 Accuracy of the Estimates

The fundamental question concerning the estimates displayed in Table 6 is whether they are sufficiently close to the true values for the evidence to be useful for policy making purposes. We believe that our results are reasonably close to the true value for a variety of reasons. In particular, we feel that the estimates of the reduction in the period of joblessness due to being placed versus being referred-but-not-placed provides a conservative estimate of the true effect.

Our best evidence for this view is that the quasi-experimental estimates of being placed relative to being referred-but-not-placed for workers most like claimants are remarkably close to the non-

experimental estimates for claimants. The quasi-experimental estimate for job-seekers with strong work records is -7.510 weeks, compared to the non-experimental estimate for claimants of -7.718 weeks. Moreover, use of a non-experimental estimator similar to the one used in this section produced estimates that were even smaller than the quasi-experimental estimators making us believe that such estimators produce conservative estimates.

We have already noted that the best way to judge the validity of the non-experimental estimates is to compare them to the results of a properly executed random-assignment design. We also made clear that we concur with the view expressed by the expert panel that the quasi-experimental evidence is not conclusive and executing a new survey would be extremely useful.

However, we also feel that it would be a mistake to take the extreme view that since we cannot be certain the quasi-experimental estimates are unbiased we should assume that they are highly biased. After all, the internal evidence in the survey suggests that our estimates for job-seekers with strong work-records would not be seriously biased even if our comparison group included job-seekers screened out by employers.[8]

The bottom-line is that we need additional quasi-experimental evidence to effectively refute claims that our estimates might enormously overstate the true value of a placement relative to a referral not leading to a placement. Nevertheless, we feel that it is highly plausible that our estimates are good first-approximations of the true effects, or put another way, it is plausible that job-matching services leading to placements have strong positive effects.

---

[8] The experts did not claim that they had evidence either that being screened out increased jobless duration or that many screened out job-seekers were in the quasi-control group used to estimate placement effects for job-seekers with strong work-records. What they claimed is that both hypotheses were sufficiently plausible that they can and should be ruled out.

We believe that it is plausible that some job-seekers with poor work records lacked good qualifications and were screened out by employers based on characteristics that are not observable directly or indirectly in our data, and that those job-seekers would have more difficulty finding work than job-seekers with similar observable characteristics. However, we also think it is plausible that an equal number are screened out because they are over-qualified and would not accept a job even if one was offered (and presumably would have less difficulty finding work than job-seekers with similar observable characteristics).

Importantly, this counter-argument only reinforces the basic point that having evidence that experts agree comes from a properly executed experimental or quasi-experimental design is invaluable because it tends to eliminate speculation about the myriad of possible ways results could be biased in one way or another.

First, it is clear that placements lead to a quick return to work. The question is how quick would have the return to work have been otherwise. Similarly, it is evident that at any point during a spell of joblessness relatively few claimants return to work within the subsequent five weeks.[9][9] Thus, over-estimates can only come about by the claimants who are placed being in the small group of claimants who would have rapidly found jobs by other means.

While we cannot prove that the non-experimental estimators eliminated much of the selection bias that was present, the comparisons presented in table 7 show that the estimators substantially modified the estimates in the appropriate direction.

Table 7. Mean Differences and Regression Adjusted Differences in Joblessness

	Referred-not-placed Versus not-referred	Placed versus Not-referred	Placed versus Referred-not-placed
Mean difference in unemployment duration	.208	-12.254	-12.462
Regression adjusted difference	-2.149	-9.867	-7.718
Effect of regression adjustment	2.357	-2.387	-4.744

Column 1 shows that the simple difference in mean duration between those referred-but-not-placed and those not-referred is .208 weeks, but when the regression model is applied the difference becomes -2.149 weeks. This implies that those not referred who most closely resemble those who were referred-but-not-placed were unemployed about 2.357 weeks longer on average. This difference is consistent with there being negative selection associated with Job Service use.

Column 2 shows that the regression model reduces the mean difference in duration between those placed and those not-referred from -12.254 weeks to -9.867 weeks. This implies that those not placed

[9] Claimants can afford to be quite selective in the jobs they interview for and accept, especially in the first 5 to 10 weeks of their unemployment spell. Indeed, a key purpose of UI benefits to allow recipients the time they need to locate the most suitable job among those available. Thus, the presence of UI payments will lead claimants to remain unemployed for substantial periods, unless they find highly suitable jobs. As claimants get closer to exhaustion, however, they are more likely to settle for jobs that they would not take earlier, but their inability to locate suitable work suggest that locating any acceptable job might be quite difficult. Thus, without Job Service placements it is plausible that they would remain unemployed for substantial periods.

who most closely resemble those placed were unemployed 2.387 weeks less than average. This reduction is in keeping with expectations that those placed would find it easier to find jobs through use of the Job Service or through other means.

Column 3 shows that use of the regression model reduces the measured effect of placements relative to being referred-but-not-placed by 4.744 weeks. This seems to us to be a large reduction that easily could be close to the amount of bias we would expect to have to take into account.[10] Indeed, we regard the resulting estimates as conservative because they assume that the entire regression adjusted referral effect is due to selection bias, while it is plausible that at least half of the effect is due to the value of viewing listings and interviewing for jobs.

A key reason for believing that referral effect are positive is that we doubt that job-seekers would waste their time using the Job Service over a considerable period if they felt that it was of no value. The data show that after the 10th week of unemployment 45 percent of claimants who are referred but not placed have actively used the Job Service in the 7 weeks prior to returning to work, and 20 percent actively used the Job Service in the preceding 12 weeks. This suggests that at least some of the measured effect represents more than a spurious correlation between use of the Job Service and otherwise unmeasured increased search intensity that is the true reason for those referred finding jobs more quickly.[11]

5.0 Estimates of Benefit-Cost Ratios

While we recognize that we cannot demonstrate that our estimates of the per-person effects of referrals and placements are unbiased, we feel that these estimates are sufficiently indicative of the true effect to be used as the basis for comparing benefits to costs. Because of the uncertainty concerning the accuracy of these estimates we use highly conservative assumptions to estimate the cost-effectiveness Job

[10] Selection bias is a major problem in most studies of the effectiveness of employment and training programs because the per-person effects are small relative to plausible values of selection bias. By focusing on placement effects, which (we argue) should be quite large, the problem is considerably lessened. While we are unaware of a systematic attempt to measure the size of the bias, we believe that studies of job search assistance suggest the bias in no larger than our estimates of the positive referral effects.

[11] An even greater persistence of use is found among placed job-seekers. Two-thirds of the claimants placed after the 10th week of unemployment had been actively using the Job Service to obtain referrals in the preceding 7 weeks, and about 20 percent had been actively using the Job Service in the preceding 12 weeks. We regard this persistence as additional evidence that the job-seekers who were placed recognized that the Job Service was providing a service of high value to them, and the measured effects should be large.

Service matching services. This evidence strongly suggests that services provided by the Job Service are highly cost-effective.

Table 8 displays our benefit-cost estimates derived from use of the quasi-experimental estimates. Line 1 shows our estimates of the reduction in joblessness derive from being placed by Job Service referrals. To translate the reduction in weeks into a dollar figure we multiple the reductions in joblessness by the average weekly earnings of the placed job-seekers subsequent to their returning to work. These calculations indicate that each placed job-seeker with a strong work record gained $1,872 in earnings and each placed job-seeker with a spotty work record gained $684.

Table 8. Calculation of Benefit-Cost Ratio Using Quasi-Experimental Estimates for Washington Placement in 1998.

	---- work record ----	
	Strong	Spotty
Reduction in weeks of unemployment	7.2	3.8
Earnings per week	$260	$180
Increase in earnings	$1,872	$684
Number placed in 1998	11,144	35,038
Total increase in earnings	$20,861,568	$23,965,992
Total earnings increase	$44,827,560	
Cost of running the PLX	$25,000,000	
Benefit cost ratio	1.8	

We then multiply the per-person earnings gains times the number of individuals placed in 1998 based on the conservative assumption that all job-seekers with strong work records were claimants. Finally, we compare the total gains from all placements, which equals $44.8 million to the cost of operating the entire Washington State labor exchange system, which is $25 million. The resulting benefit-cost ratio is 1.8, which is quite respectful for any government sponsored employment and training program.

We regard these estimates as conservative because the entire cost of running the Job Service is being compared to the value of placements alone. Yet as shown in table 1a, 60 percent of registrants receive some reportable services, while less than 10 percent of registrants are placed. Thus, even if the benefits derived from referrals not leading to placements, workshops and other forms of job search

assistance, various types of counseling and testing are very small, there total value would be quite high because they affect about 200,000 people.

The key weaknesses in these estimates, however, is that we apply estimates derived from a small sample that may not be representative of the universe to all those placed, and the estimates for job-seekers with spotty records may be over-estimated. Nevertheless, benefits equal costs even if we assume the benefits to job-seekers with spotty work records are zero, as long as the omitted benefits are slightly greater than zero.

Table 9 shows similar calculations using the per-person reductions in joblessness derived from the non-experimental estimates for UI claimants for 1995. The Oregon estimates are identical to the ones shown in table 3, but the Washington estimates are based on those that apply to 1994.[12] These figures are a bit lower than the average figures shown in table 3, but reflect economic conditions much more similar to those in 1995.

We then multiplied the reduction in joblessness by the average weekly earnings in the year following the return to work to obtain estimates that on average each referral not leading to a placement increased earnings by $272 in Oregon and $416 in Washington, while each placement increased earnings by $1,136 in Oregon and $1,716 in Washington.

Finally, we multiplied the per-person earnings gains by the number of claimants in the databases we used to produce the per-person estimates of the reduction in joblessness. This produced estimates that referrals plus placements increased earnings by $19.5 million and $24.3 million in Oregon and Washington, respectively.

The Oregon figure equals about 80 percent of the cost of running its Job Service in 1995, and the Washington figure is about 10 percent greater than the cost of running its job service. However, because our estimates are limited to claimants, we feel that it is reasonable to compare the benefits to the cost of helping claimants alone. We made a rough estimate that 38 percent and 25 percent of the entire cost of running the Job Service in Oregon and Washington, respectively, goes to helping claimants. This

[12] We did not use Washington results for 1995, itself, because we felt that these estimates were not very accurate because we could not follow the return to work for a long enough period to properly exclude labor force dropouts from the comparison group.

Table 9. Calculation of Benefit-Cost Ratio Using Non-Experimental Estimators for Claimants in Washington and Oregon during 1995.

	Oregon	Washington
Reduction in weeks of unemployment		
per-referral	1.1	1.6
per-placement	4.6	6.6
Earnings per week	$247	$260
Increase in earnings		
per-referral	$272	$416
per-placement	$1,136	$1,716
Number (according to administrative data)		
Referred in 1995	34,704	34,648
Placed in 1995	8,876	7,533
Total increase in earnings		
Due to referrals	$9,429,225	$14,413,574
Due to placements	$10,084,763	$12,926,563
Total earnings increase	$19,513,988	$27,340,137
Cost of running the PLX	$26,000,000	$24,300,000
Percent spent on claimants	38.0%	25.0%
Benefit cost ratio		
Due to placements	1.02	1.26
Due to referrals and placements	1.98	2.62
Calculation of Percent of Funds Spent on Claimants		
Number (according to official statistics)		
Referrals in 1995	143,697	218,068
Claimant referrals in 1995	54,605	54,517
Claimant placements in 1995	13,579	11,524
Claimant referrals as percent of all referrals	38.0%	25.0%
Official referral as percent of administrative referrals		
Official placements as percent of administrative referrals		

estimate is based on using official statistics to compare the ratio of all referrals to referrals made to claimants.

Making these adjustment to cost lead to obtaining benefit-cost ratios of 1.02 and 1.26 for Oregon and Washington placements, respectively. The benefit-cost ratios increase to 1.98 and 2.62 in Oregon and Washington, respectively, when both referrals and placements are included. The Washington benefit-cost ratios are about 25 percent greater than those for Oregon both because benefits are greater and costs are less.

However, the administrative data we used did not include all the referrals and placements reported for claimants in official statistics. We believe that much of the difference is due to our counting referrals and placements made only while claimants were collecting benefits, but the official statistics count as claimants those who exhausted benefits. It also appears that our Oregon database omitted substantial numbers of claimants, possibly because claimants who established a benefit year in 1995 and then collected benefits in that benefit-year in calendar-year 1996 were omitted from the file.

If we reduce the cost to reflect the omission of services delivered to those claimants the benefit-cost ratios increase by over 50 percent in both states. Thus, we conclude that the benefits received by placed claimants alone are substantially greater than costs of providing those services, and would be greater still if all placements going to claimants were included in the datasets. Adding the benefits stemming from referrals not leading to placements increase the benefit-cost ratios substantially, and would do so even if selection and other measures reduced the true effect by large amounts.

5.1 Additional Benefits and Costs

The above benefit-cost calculations focus on estimation of the largest benefits—increased earnings, but these estimates do not provide estimates from the viewpoint of different members of society. In particular, they do not examine the loss of UI benefit payments that referred and placed claimants would have otherwise received or the possibility that the gains to these individuals come at the expense of a slower return to work by job-seekers who did not use the Job Service. We, therefore, examine reductions in UI payments and possible crowding-out effects in this section.

Table 10 displays the calculations needed to estimate reductions in UI payments. The first two lines show the estimated effect of referrals and placements on the number of UI payments. The next line shows the average payment, which is roughly half of the average weekly earnings of the claimants. We then multiply the per-person reduction in payments due to referral and placements times the average payment to estimate the per-person reduction in payments. Next we show the number of referrals and placements, which are identical to the figures in table 9. We then show the total reduction in payments by multiplying the incidence of referrals and placements times the per-event reductions. Finally, we compare the reductions in payments to the increase in earnings displayed in table 9, and the cost of providing services to claimants.

Table 10. Calculation of Reductions in UI Payments- Using Non-Experimental Estimators for 1995 in Washington and Oregon during.

	Oregon	Washington
Reduction in weeks of benefit payments		
per-referral	0.00	0.05
per-placement	2.30	3.13
Average weekly UI payment	$124	$130
Decrease in payments		
per-referral	$0	$6
per-placement	$283	$407
Number (according to administrative data)		
Referred in 1995	34,704	34,648
Placed in 1995	8,876	7,533
Total decrease in payments		
Due to referrals	$0	$326,012
Due to placements	$3,847,880	$4,684,621
Both	$3,847,880	$5,010,633
Total decrease in payments as a Percent of the increase in earnings		
Due to referrals	0%	2.3%
Due to placements	38.2%	36.2%
Both	19.7%	18.3%
Total decrease in payments as a Percent of costs of serving claimants		
Due to placements	38.9%	77.1%
Due to referrals and placements	38.9%	82.5%

Overall, the reduction in UI payments equaled a bit less than 20 percent of the increase in earnings in both states. The reduction in UI payments is considerably less than the increase in earnings because: (a) payments equal only about one-half of earnings, and (b) payment reductions end at the point claimants exhaust benefits. Because many referrals and placements occur close to benefit exhaustion and those referred and placed claimants otherwise would be unemployed for long periods after exhaustion the per-person increase in earnings is more than twice as great as the per-person decrease in payments.

It also is interesting that placements have a large effect on UI payments, equaling about 37 percent of earnings reductions, but referrals have virtually no effect. This result provides further evidence that during the period benefits are paid claimants are quite reluctant to settle for any but highly suitable jobs. Thus, placements have a very strong effect presumably because they very well match the skills and interests of the claimants, but referrals not leading to placements appear to only have a strong effect as claimants have searched for long periods and begin to realize that they will have to settle for less desirable jobs than they hoped to find.

Finally, it is noteworthy that the reduction in UI payments comes within about 80 percent of the cost of providing services to all claimants, even those who have exhausted benefits. The reduction in UI payments comes close to 40 percent of the cost of serving claimants in Oregon. The smaller percentage is largely due to our allotting a much larger fraction of total costs to serving claimants, and Oregon using its own funds to supplement its Wagner-Peyser grant.

Another issue examined in our report was the extent to which the quicker return to work by claimants helped by Job Service referrals and placements slowed the return to work of job-seekers who did not receive this help. These crowding-out effects are potentially important because it could be the case that the benefits going to Job Service users largely come at the expense of other job-seekers. If this was the case the benefits in terms of reductions in UI payments would represent the major benefit to society, but the primary effect would simply be a redistribution of unemployment among job-seekers.

The crowding-out analysis was performed by Professors Carl Davidson and Stephen Woodbury of Michigan State University (and in Dr. Woodbury's case the Upjohn Institute). They used the Washington State estimates displayed in table 4 along with a variety of other data as parameters in a model they developed designed to measure the effect of various government interventions on total

unemployment. They concluded that under a wide range of plausible assumptions the primary effect is a small decrease in unemployment (of about 0.08 percentage points), and a much smaller crowding-out effect (of about 0.061 weeks, which is an increase in duration of roughly 0.4 percent).

That the reduction in unemployment is small is hardly surprising, given that the number of placements made by the Job Service represents a small fraction of all hires. However, the decrease is large enough to conclude that only 20 percent of the gains in earnings shown in table 9 come at the expense of other workers.

Overall, we conclude that the primary beneficiaries of Job Service referrals and placements made to claimants accrue to the claimants themselves. The benefits are especially large per-person for those placed, even though about 40 percent of their gains in earnings comes at the expense of reductions in UI benefit payments. The benefits stemming from referrals are much smaller per-person, but large in total because about five times more claimants are referred than placed. Also, all most none of the earnings gains are offset by losses of UI benefit payments.

Job-seekers who do not use the Job Service suffer very tiny per-person losses. However, because the number of job-seekers affected are large the total reduction in earnings equals about 20 percent of the gain experienced by placed and referred claimants.

Society at large and employers also gain from reductions in UI benefit payments and the reduction in joblessness. UI benefit payment reductions alone come close to covering the costs of providing services to claimants in Washington, and cover a substantial proportion of the costs in Oregon. It is difficult to say how the benefits are distributed between employers and consumers because it is unclear how much of the cost of paying UI benefits is passed on to consumers. A reasonable guess is that the costs are split evenly between the two groups. Quantifying the benefits of reduced unemployment also is difficult, but even the benefits of the small reduction is likely to cover a large fraction of the costs of providing the benefits.

6.0 Summary

The expert panel that reviewed our research agreed that the results summarized in table 11 provide the best available estimates of the per-person reductions in joblessness and UI benefit payments resulting from Job Service referrals and placements. At the same time, they felt that the results are far from definitive because the quasi-experimental approach did not rule out the possibility that the estimates are biased because members of the quasi-control group were screened-out by employers and the sample may have been too small and unrepresentative of the universe.

Thus, the panel concluded that the primary achievement of this work was to describe a procedure that could overcome the formidable estimation problems stemming from an inability to apply a random-assignment design. The panel felt that the best course would be to execute an improved survey, before the non-experimental results should be accepted as valid.

The authors concur with the expert panel that it would be of enormous value to clear-up the issues they raised by conducting an improved survey. We are especially concerned about the validity of our results for job-seekers with spotty work records. However, we feel that the expert panel was taking an extreme position with respect to the results for claimants. Our analysis provided substantial evidence that the quasi-experimental results were not substantially biased by employer actions, and that any bias in the non-experimental results were small relative to the magnitude of the placement effects.

In the absence of empirical information to the contrary, we therefore believe that our placement effect estimates that assume the positive referral effects solely are due to measurement error represent conservative estimates of the true effect. Based on this evidence it appears that the earning gains experienced by placed claimants more than cover the cost of providing services to this group.

Also, because the costs of providing services are so small per-person, only a tiny fraction of our measure of the benefits of referrals not leading to placements need to represent the effect of these services rather than measurement bias. In addition, we assumed that no benefits accrue from any other service provided by the Job Service. This makes us believe that the total benefits are highly likely to substantially exceed costs.

Table 11. Summary of the Study Characteristics and Measures of PLX Benefits

Data source	Population studied	Back to work effect of: Placement relative to referral	Back to work effect of: Referral relative to no referral[1]	Total PLX benefits per year[2]	Benefit – cost comparisons[3]
Study-1 Washington Mail Survey and Administrative Data for the first half of 1998	A sample of 587 individuals referred to PLX job openings	7.2 weeks sooner for job seekers with strong work records 3.8 weeks sooner for job seekers with weak work records	Not examined	$45 million for all 1998 PLX users from placements alone	Annual cost $25 million Benefit-cost ratio 1.8
Study-2 Washington Administrative Data for 1987–95	A sample of 328,815 spells of unemployment experienced by UI claimants	7.7 weeks sooner	2.1 weeks sooner	$11 million for claimant placements alone 1987-95 $25 million for claimant placements and referrals 1987-95	Annual cost $25 million 35 percent spent on claimants Benefit-cost ratio between 1.2 and 2.8
Study-3 Oregon Administrative Data for 1995	A sample of 138,280 spells of unemployment experienced by UI claimants	4.6 weeks sooner	1.1 weeks sooner	$15 million for 1995 claimant placements alone $30 million for 1995 claimant placements and referrals	Annual cost $26 million 38 percent spent on claimants[4] Benefit-cost ratio between 1.6 and 3.1

[1] Referral effects measure the value of information obtained by viewing PLX listings and obtaining staff aid that improves the decision making of placed and non-placed PLX users.

[2] Study 1 uses published statistics to estimate the number of placements. Study 2 uses tabulations of person-level files to measure the number of placements and referrals. Study 3 uses both sources of information. Use of published data for 1995 raised benefit estimates for Study 2 to $42 million for placements and referrals together and $13 million for placements alone. This increased the 1995 benefit-cost ratios to 4.5 for placements and referral and to 2.1 for placements alone.

[3] Benefit-cost ratios are not adjusted for crowding-out effects. Their inclusion would reduce the ratios by about 20 percent.

[4] Only 25 percent of Washington PLX costs went to referring claimants in 1995.

Acknowledgements

This paper is based on research reported in Office of Workforce Security, Employment and Training Administration, U.S. Department of Labor, Occasional Paper 2000-06: Measuring the Effect of Public Labor Exchange (PLX) Referrals and Placements in Washington and Oregon, October 2000.

An unusually large numbers of organizations and individuals contributed to this project and merit our thanks. First, and foremost we are grateful to the United States Employment Service Office of the Employment and Training Administration, U.S. Department of Labor for supporting this work through a grant to the Washington State Employment Security Department.

Our primary contact within ETA was David Balducchi who provided invaluable aid and support. Equally important was the help we received from Jeff Jaksich in the "other" Washington. In addition, the Oregon Employment Department provided extraordinary support by processing its data to our specifications. Patrick McIntire was our primary contact in Oregon.

We are also grateful for the comments we received from our panel of technical experts: Burt Barnow, Johns Hopkins University; Arnold Katz, University of Pittsburgh; Bob LaLonde, University of Chicago; Jeff Smith, University of Western Ontario; Dan Sullivan, Federal Reserve Bank of Chicago; and Steve Woodbury, Michigan State University and the Upjohn Institute. We are also grateful to Dr. Woodbury and his colleague Carl Davidson at Michigan State University for executed the analysis of crowding out effects.

Finally, we are grateful for the assistance we received from many of our colleagues at Westat for their help in executing the analysis and preparing the report for publication.

Despite all of the help we received from others, any errors in the work are the responsibility of the principal authors, and the opinions expressed in this paper are solely those of the authors.

CE 083412

U.S. Department of Education
Office of Educational Research and Improvement (OERI)
National Library of Education (NLE)
Educational Resources Information Center (ERIC)

ERIC Educational Resources Information Center

REPRODUCTION RELEASE

(Specific Document)

I. DOCUMENT IDENTIFICATION:

Title: Measuring the Effect of Job Service Referrals and Placements in Washington and Oregon	
Author(s): Louis JACOBSON & Ian PETTA	
Corporate Source: Westat Inc	Publication Date: OCT 2000

II. REPRODUCTION RELEASE:

In order to disseminate as widely as possible timely and significant materials of interest to the educational community, documents announced in the monthly abstract journal of the ERIC system, *Resources in Education* (RIE), are usually made available to users in microfiche, reproduced paper copy, and electronic media, and sold through the ERIC Document Reproduction Service (EDRS). Credit is given to the source of each document, and, if reproduction release is granted, one of the following notices is affixed to the document.

If permission is granted to reproduce and disseminate the identified document, please CHECK ONE of the following three options and sign at the bottom of the page.

The sample sticker shown below will be affixed to all Level 1 documents

PERMISSION TO REPRODUCE AND DISSEMINATE THIS MATERIAL HAS BEEN GRANTED BY

Sample

TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)

1

Level 1

☒

Check here for Level 1 release, permitting reproduction and dissemination in microfiche or other ERIC archival media (e.g., electronic) and paper copy.

The sample sticker shown below will be affixed to all Level 2A documents

PERMISSION TO REPRODUCE AND DISSEMINATE THIS MATERIAL IN MICROFICHE, AND IN ELECTRONIC MEDIA FOR ERIC COLLECTION SUBSCRIBERS ONLY, HAS BEEN GRANTED BY

Sample

TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)

2A

Level 2A

☐

Check here for Level 2A release, permitting reproduction and dissemination in microfiche and in electronic media for ERIC archival collection subscribers only.

The sample sticker shown below will be affixed to all Level 2B documents

PERMISSION TO REPRODUCE AND DISSEMINATE THIS MATERIAL IN MICROFICHE ONLY HAS BEEN GRANTED BY

Sample

TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC)

2B

Level 2B

☐

Check here for Level 2B release, permitting reproduction and dissemination in microfiche only.

Documents will be processed as indicated provided reproduction quality permits.
If permission to reproduce is granted, but no box is checked, documents will be processed at Level 1.

I hereby grant to the Educational Resources Information Center (ERIC) nonexclusive permission to reproduce and disseminate this document as indicated above. Reproduction from the ERIC microfiche or electronic media by persons other than ERIC employees and its system contractors requires permission from the copyright holder. Exception is made for non-profit reproduction by libraries and other service agencies to satisfy information needs of educators in response to discrete inquiries.

Sign here, → please

Signature: Louis Jacobson	Printed Name/Position/Title: LOUIS JACOBSON Senior economist
Organization/Address: Westat, Inc.	Telephone: 301 251 8229 FAX:
	E-Mail Address: LOUISJACOBSON@WESTAT.COM Date: 30 JUN 03

(Over)

III. DOCUMENT AVAILABILITY INFORMATION (FROM NON-ERIC SOURCE):

If permission to reproduce is not granted to ERIC, *or*, if you wish ERIC to cite the availability of the document from another source, please provide the following information regarding the availability of the document. (ERIC will not announce a document unless it is publicly available, and a dependable source can be specified. Contributors should also be aware that ERIC selection criteria are significantly more stringent for documents that cannot be made available through EDRS.)

Publisher/Distributor:
Address
Price:

IV. REFERRAL OF ERIC TO COPYRIGHT/REPRODUCTION RIGHTS HOLDER:

If the right to grant this reproduction release is held by someone other than the addressee, please provide the appropriate name and address:

Name:
Address:

V. WHERE TO SEND THIS FORM:

Send this form to the following ERIC Clearinghouse: **Cheryl Grossman**
Processing Coordinator
ERIC Clearinghouse on Adult, Career, and Vocational Education
Center on Education and Training for Employment
1900 Kenny Road
Columbus, OH 43210-1090

However, if solicited by the ERIC Facility, or if making an unsolicited contribution to ERIC, return this form (and the document being contributed) to: